

the growth of scientific knowledge. The facts discovered in the laboratory have been turned to account in the alkali work, and the theories of the chemist have not unfrequently received confirmation at the hands of the manufacturer. Conversely, the wants of the manufacturer have hastened the discovery of fresh facts, and the success or failure in the application of these facts on the large scale has reacted beneficially upon the advance of chemical theory. In 1750 sulphuric acid was manufactured by distilling sulphate of iron in earthen vessels luted to glass receivers. The destruction of plant obliged the manufacturer to adopt a better method. The chemist supplied him with the facts: Nitre and sulphur when burnt together produced sulphuric acid. The manufacturer supplied the mechanical means for realising this process on the (comparatively) large scale. Soon after this time Scheele discovered chlorine; the manufacturer, acting on the experiments of the chemist, turned to account the fact that chlorine readily combines with hydrogen. But the impulse thus given to the bleaching trade necessitated a corresponding advance in the manufacture of sulphuric acid. The chambers in which the nitre and sulphur were burnt were enlarged, improvements were adopted, and the price of the acid decreased while the consumption increased.

In more modern times we see the need of a cheaper method for manufacturing chlorine, giving rise to the successful process of Weldon, a process based upon strictly experimental laboratory data, and to the hitherto not so successful process of Deacon. We see the failure of the latter process inducing its discoverer to extend his researches, and as a consequence chemical science is enriched with a valuable paper which throws considerable light upon the general principles of chemical dynamics.

While the history of the alkali trade illustrates the benefits conferred upon manufactures by science, and the requital made to science by manufactures, it also forcibly illustrates the uses which to-morrow may bring for the waste products of to-day.

The monopoly granted by the King of Sicily to one firm in the exportation of sulphur obliged the manufacturers of oil of vitriol to have recourse to some other source of sulphur. The introduction of pyrites led to the accumulation of burnt ore, and this again to Henderson's method for extracting copper, a method which, whether considered chemically or commercially, has proved most successful. The hydrochloric acid sent out from the chimney of the alkali works has, since 1863, been almost wholly condensed, and from this once wasted acid immense quantities of bleaching powder are now manufactured. The acid liquors from the manganese still, although rich in manganese, were formerly sent into the nearest stream, thus causing at once a loss to the manufacturer and a nuisance to the neighbourhood. Now, however, these liquors are turned to use, the nuisance is abated, and the manufacturer is enriched.

But if one is to acquire a just idea of the immense dimensions, and of the importance of the alkali trade from a commercial, chemical, or general point of view, one must learn in detail the history of the manufacture, the development of the processes which gather round the alkali trade as their centre, and the connections

which subsist between the practical carrying out of the manufacture and the general principles of chemical science. Such a knowledge may be obtained from the work before us. Mr. Kingzett gives a clear and succinct account of the rise of the alkali trade and of the present state of the manufacture. Notices, sufficiently detailed for the purposes of the general reader, of all the recent improvements are introduced. The allied trades, especially the bleaching powder and soap manufactures, are described.

The book necessitates a general knowledge of chemistry on the part of the reader, inasmuch as processes are everywhere referred to their fundamental chemical principles. He who wishes for a rule of thumb acquaintance with the alkali manufacture will certainly find the information given in this work beyond his scope. On the other hand, the man who, having a general knowledge of chemistry, really wishes to learn how chemical facts are turned to account in manufactures, and also how mechanical difficulties are overcome, cannot do better than study—not read only—the work before us.

The chemical manufacturer also may gain from this work a more extended knowledge of his trade, and he may receive many hints, which, if he be of an inventive turn of mind, he may some day turn to account. The author has evidently endeavoured to treat the subject from the standpoint of the scientific manufacturer, and we think he has very fairly succeeded.

Full details of the more modern improvements of Hargreaves, Mactear, and others in the manufacture of alkali are given. The Weldon process for manufacturing bleach is described minutely, the improvement suggested by Mr. Weldon, whereby loss of calcium chloride would be avoided, is mentioned, and its utility is pointed out.

Of course there are parts of the book which it appears to us admit of improvement. The introduction of an index would add to the value of the work. Might we suggest to Mr. Kingzett that it would be well to re-write the preface, and generally those portions of the work in which he indulges in philosophising? The book begins with a platitude: "The wealth of a nation may be said to be indicated by the magnitude of its commerce." It closes (the last chapter is purely statistical) with a poor simile: "Life may be compared to a spectrum with its bright and dark lines."

M. M. PATTISON MUIR

OUR BOOK SHELF

River Terraces. By Col. George Greenwood. (London: Longmans and Co.)

FOR somewhere about fifteen years no name occurred more frequently in the geological correspondence of our magazines and newspapers than that of "George Greenwood, Colonel," and no letters carried with them a more marked individuality than those to which that name was appended. They never betrayed any doubt or hesitation, but made merry over the doubts and difficulties of other and more experienced observers; they showed in vigorous language that in so far as a correspondent agreed with their author, he was right, that in so far as he differed he was wrong. Fathers in science like Lyell and Darwin, as well as striplings, not yet emancipated from geological long-clothes—one and all needed instruction and correction at the hands of the enthusiastic Colonel. He spoke of the

geologists of the country as schoolboys, whom he had to drill in the beggarly elements, and divided them into classes according to their acquirements or their aptitude to receive his lessons. He began by enforcing his views as those of Hutton and Playfair, and gradually so identified himself with them that he regarded them and the very words expressive of them as his own property, which no one should claim or touch except in the way he chose to sanction. Peace be with his memory! He did a good work in his time. Men gladly overlooked his personal failings for that sound sense so often underlying his self-asserting remarks about geological forces which had not been adequately understood in this country when he began his crusade of "Rain and Rivers." The present volume is a reprint of his letters on all manner of subjects, written at different times from 1859 to 1875. But surely its publication was not needed for the scientific reputation of the author. The letters are given as they originally appeared, full of references to passing incidents, and to letters by other writers, which of course are not inserted, but without which Col. Greenwood's diatribes are often unintelligible. There is no attempt at editing. The title of the book also is misleading. Instead of a treatise on river terraces, it is a medley of clippings from the columns of various periodicals relating to such varied subjects as Spelling, the Possessive Augment, Source of the Nile, Glen Roy, a Horse-Chestnut Tree, Rain and Rivers, Sirluin, Pronunciation of Latin, Lakes with Two Outfalls, a Beech pierced by a Thorn Plant, Origin of the Chesil Bank, &c., &c.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications. The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Tait on Force

IN Prof. Tait's lecture on "force," which its writer seems to have intended as a model of perspicuity and accuracy, we are told that "we must measure a force by the rate at which it produces change of momentum." Nothing could be clearer or more satisfactory than this statement. Then Prof. Tait proceeds to tell us what force "is," and we read—"Force is the rate of change of momentum"—giving to this word "is" the meaning which the so-called metaphysicians give to it; and it seems to me that we might jangle over it for ever, without ever knowing whether this latter statement be true or not; for although we may all agree as to the proper *measure* of a force, it seems to be more difficult to tell what force "is." Possibly we might roughly measure the hunger of a man under different circumstances, by determining the number of pounds of beef he would consume, but it would be hardly warranted to say that hunger "is" a certain number of pounds of beef.

Perhaps it may be advantageous to apply the name force to the thing which we have heretofore called rate of change of momentum due to force, but I cannot imagine how any one can think that a certain "rate of change of momentum" can produce a unit of momentum in a unit of time. Until this shadowy "phantom" called force can be brought a little more sharply into focus, it seems to me that considerations as to what it "is" may profitably be left to those who appear to delight in the obscurity of obscure things—the metaphysicians.

St. Louis, June 4

FRANCIS E. NIPHER

P.S.—On showing this note to a friend, my attention was drawn to a note by Robert Napier in *Engineering*, which makes the present one seem almost superfluous. Remembering, however, the difficulty which I had in understanding these very points on account of the loose way in which they are put in many text-books, I feel that too much cannot be done to prevent such things from going into text-books in the future.—F. E. N.

[Prof. Nipher's censure does not apply to my lecture, simply because he fails to remark that I had two objects in view, (1) to point out the sense in which the word *force* must be used if we desire to avoid confusion; (2) to point out that, in all probability, there is no such *thing* as force. Under the first head I of course referred to Newton's "Laws," and in them language is used which at least suggests the objective reality of force as the *cause* of change of motion. We must take Newton as we find him. But there is no inconsistency in afterwards proceeding to give reasons which appear conclusive against the objective reality of force.]

With some of Prof. Nipher's other remarks I can cordially agree. Since my lecture was published I have been in almost daily receipt of passages containing errors amounting often to the wildest absurdities, due to misuse of the term force. The latest to which my attention has thus been called is in the *Cornhill Magazine* for June. Here the non-scientific public is gravely told that "*what mathematicians call the moving force exerted by the earth on the moon is eighty-one times greater than the corresponding force exerted by the moon on the earth.*"—P. G. T.]

On Time

If I understand V. A. Julius's letter in *NATURE*, vol. xiv. p. 122, on the measurement of time, it may be thus summarised:—

As equal times, unlike equal linear magnitudes, cannot be brought alongside of each other, their equality can be ascertained only by means of velocities. (This will not be disputed.) We define those times as equal during which the same space is traversed by equal velocities; but the postulate that a velocity, *e.g.* that of the earth's rotation, continues unchanged, is arbitrary, incapable of proof, and justified only by practical convenience.

It seems to me, on the contrary, that the postulate is not necessarily arbitrary, but may be absolutely justified by facts. The best case to put is that of the pendulum, which, according to Sir William Thomson, is probably capable of measuring time with greater accuracy than the motion of the earth itself. If we assert that equal forces acting through equal spaces produce equal velocities (and this is rather a definition than an axiom), then the assumption of the equal velocity of all the pendulum's strokes postulates nothing except that the force of gravitation continues unchanged. I admit that I see no way of proving this, but it may be safely assumed in the absence of any known or probable cause of change.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, June 19

The Antiquity of Man

I HAVE no desire to enter into the controversy respecting the age of the palæolithic implements found in brick earth near Brandon, by Mr. Skertchly. I had the great pleasure of going over some part of the ground with him and Mr. Belt in November last. But what I saw then was not sufficient for me to make up my mind upon the question. Of course Mr. Skertchly, with his immense experience, has far more evidence in his repertory than a cursory visit could afford to me.

My object in writing this letter is to point out that, if it should eventually be proved that a glaciation of the surface has occurred in East Anglia subsequent to its human occupation, but one which is not identical with, but posterior to, that glaciation (whether land or marine) which deposited the great chalky boulder-clay, then this is no more than I put forward many years ago in my papers on the "warp" (*Geol. Journal*, 1866); and on the "glacial origin of denudation" (*Geol. Mag.*, 1866); and on the "denudations of Norfolk" (*Geol. Mag.*, 1868).

I think this is the direction to which the course of opinion appears to be tending, and I ask you kindly to bring under the notice of the younger generation of geologists the speculations of an elder brother.

I call the product of this supposed glaciation "trail." The more orthodox, I believe, consider it to be "rain wash," and I had hoped that some competent writer would have thought me worth confuting. But none has done so. I have reason to think that one of your correspondents did actually put down as boulder clay this very deposit, at one of the most important sections which I saw near Brandon.

O. FISHER

Harlton, Cambridge, June 28